

**MARBLED MURRELET MODULE BLIND PEER REVIEW RECONCILIATION  
REPORT (CHAPTERS 1, 2, 3, & 5) MARCH 18, 2005**

**NORTHWEST FOREST PLAN – THE FIRST 10 YEARS (1994-2003): STATUS  
AND TREND OF POPULATIONS AND NESTING HABITAT FOR THE  
MARBLED MURRELET**

**TECHNICAL COORDINATOR  
M.H. HUFF**

**LEAD AUTHORS  
M.H. HUFF, S.L. MILLER, S.K. NELSON, AND M.G. RAPHAEL**

Our reconciliation report is organized by Chapters 1, 2, 3, and 5, followed by Summaries and Abstract. Reconciliation for Chapter 4 will be submitted separately at a later time. We replied to each reviewer separately.

**Chapter 1**

**Reviewer #1**

Perhaps in this introductory chapter, describe what portions of the Marbled Murrelet effectiveness monitoring approach are not addressed by this 10-year report, and if they will be addressed in future monitoring cycles.

**Reply:** The following new text was provided:

The steps outlined in these approaches were followed where sufficient budgets were provided. The initial investments of nearly \$1.6 million proposed in Madsen et al. (Table 5) to refine habitat definitions and identify key habitat variables and \$1.1 million to validate habitat maps, for the most part, did not materialize. Assessments of population productivity (i.e., post breeding surveys of juvenile-to-adult ratios) were not funded at levels to provide reliable information over time and thus were dropped from the monitoring program.

**Reviewer #2**

**General comments**

Chapter 1 gives a good explanation of the Forest Plan and its goals, and sets the scene for the chapters which follow. I've mentioned the need for a strong

synthesis chapter in my general comments. This introductory chapter does not provide that and is probably not where it should be – better in a final separate synthesis chapter.

**Reply:** The following new text was provided:

Plan monitoring on Marbled Murrelets is synthesized in a companion publication (Raphael et al., in review).

Chapter 1 needs to provide a map and description of the six conservation zones for Marbled Murrelet within the NWFP. These zones are mentioned in some of the other chapters, but there is no map showing where their boundaries are.

**Reply:** An additional figure was provided with the conservation zones.

### **Reviewer #3**

Reviewer #3 did not have comments on this chapter.

## **Chapter 2**

NOTE: All of the suggested edits from Reviewers 1-3 were incorporated into Chapter 2 except as described below.

### **Reviewer #1**

Overall, this chapter was very well-written and very thorough. I was impressed with how extensively and carefully the author reviewed the literature, including publications that are still in review. As you will see, most of my comments pertain to information that is very recent that possibly could be added before this report is finalized.

Abstract-p.3. It says "Few associations with respect to topographic features, such as elevation, slope, aspect, and distance to marine waters, have been found". This contradicts what is said in the body of the chapter. I would change "few" to "a number of associations have been found" and describe what those are.

Under Access, p. 18--The author might want to add that some studies in southern Oregon, California, and British Columbia have found high contrast edge around old-growth patches (e.g., from recent clearcuts) to be predictive of use of occupied or nest sites (Meyer and Miller 2002, Meyer et al. 2002, Zharikov et al. in press for Landscape Ecology). Whether this is due to site fidelity (continued

use after recently clearcut) or preference because of easier access is unknown. The Ripple et al. (2003) Oregon study did not find this to be true when habitat polygons included mature and old-growth forest. Nests near high contrast edge also had higher success in British Columbia, but that success may decline as the adjacent clearcut matures (Zharikov et al. in press). This agrees with the finding in California that, after an initial time lag, the birds eventually abandon sites following stand fragmentation by clearcuts.

**Reply:** These statements do not belong under Access and are addressed elsewhere in the paper. The first sentence is not accurate with respect to the Meyer and Miller and Meyer et al. papers.

Under Distance to Coast--p. 26. One might want to add that maximum distance to coast of occupied sites gets shorter as one moves south in the plan area.

**Reply:** This is not true as birds in Alaska travel very short distances inland because habitat is available only along the coast.

Under Nest success--p. 29. Huettmann et al. is still in review as a Wildlife Monograph submission and it states there was no association between any slope, aspect, or elevation variables with nest success.

Under At-Sea Habitat Associations--p. 31. Change Meyer et al. 2002 to Miller et al. 2002 because the latter discusses associations of offshore murrelet populations, not the former.

**Reply:** Meyer et al. 2002 was deleted from one location on this page but kept in another. This paper addressed the distribution of birds relative to seascape patterns.

## **Reviewer #2**

In general this is a good review which covers a huge amount of information concisely. A few relatively minor problems could improve the review.

First, the review often switches from discussing the biology or habitat use of the murrelet across its entire range to the more narrow 3-state situation. Sometimes it isn't obvious which sections apply to the entire range and which only to the southern states. This is important because some of the more general statements do not apply to BC or AK.

**Reply:** Rectified this by adding region when necessary for clarification.

Second, another problem is that patterns are often discussed with somewhat selective citation of certain studies and ignoring others. Several of the habitat

associations discussed were reported in some studies but not in others, and some studies even showed opposite effects. Obviously you can't verbally review all the papers, but it would be useful to get some sense about how consistent these patterns are across all studies. One way might be to present summary tables (as was done in Burger 2002 and McShane et al. 2004), or at least mention (for some commonly investigated measures) that x% of studies show this trend in WA, OR and CA and cite these reviews.

Here are a few places where this might apply:

- P. 15 bottom. The choice of these papers is somewhat selective and there are many others on these topics (see reviews Burger 2002, McShane et al. 2004).
- P. 17 whole page. Similar issue.
- P. 24 Slope, aspect and moisture regimes.

**Reply:** This manuscript was not meant to review every paper ever published. We have included many instances where results disagree. There was not time to go back and determine the percent of studies that showed a specific result (this could take many months to do).

- See also some of the specific points below where some contradictions are mentioned.

**Reply:** Addressed these.

Third, it appears that this review was written some time ago and not recently updated or revised. Many of the papers cited as in press or in review have long been published, and some cited as in press might not have been accepted (e.g., Huettmann et al. 2003 is not published). A thorough update of the references is needed.

**Reply:** Updated with some new papers. Updated lit cited.

1. p. 2, p. 7, and elsewhere. Non-migratory and remain near nests year-round. This is true only for the WA, OR & CA population (small portion of the overall population) – make this clear. There is good evidence for migration in BC and AK, although not well documented quantitatively.

2. p. 3 line 3 and elsewhere. "Cover" in this context refers to foliage cover above the nest and should be made clear.

3. p. 3 lines 7&8. This statement is not correct. There are numerous studies which report positive or negative associations with these habitat parameters (see reviews by Burger 2002, McShane et al. 2004).

4. p. 4 line 7 50-90% habitat has been lost in WA, OR and CA (I assume this is what you mean here?).

5. p. 4 end of para 1. In Canada, the murrelet is listed as threatened federally – not provincially. It is on the “Red List” in BC which is not the same thing. The Rodway reference is to the federal listing.

6. p. 6 line 4. Many (most?) coastal waters in which murrelets forage are not stratified but well-mixed by tides or by ocean currents.

7. p. 7 end para 2. Fjord waters are usually deep, not shallow. All the references you cite here show that murrelets move into sheltered inside waters (e.g., Puget Sound, Georgia Strait) which are not strictly fjords.

8. p. 8 middle. Ground nesting has been documented in southern BC which is in the middle of the species range. Might be better to state that a small proportion nests on cliffs within the mid-range (DeGange 1996, Bradley and Cooke 2001).

9. p. 8 near bottom. Stating that adults return to nests 1-12 times a day is misleading. This refers to frequency of chick meals. Each adult returns far less than this, and it is more meaningful to give a mean or mode in addition to the range.

10. p. 11 line 6 “abundance of substrate and cover”. Most studies define potential platforms purely on the basis of estimated limb or deformity diameter and height above ground, without assessing cover or epiphyte cover (which is impossible to correctly assess from the ground). Here and a few lines down specify “foliage cover above and around the nest”.

11. p. 14 para 2. Mention that these are mean dimensions of the nests. Throughout this paragraph be careful to avoid misleading references to locations. The Burger et al. 2000 and Rodway and Regehr 2002 studies were not done on the Sunshine Coast although this paragraph makes it sound like they were – very different vegetation regimes.

12. p. 17 line 2 from bottom. Change density to densities.

13. p. 19 lines 6& 7 from bottom. “sites being too open ....predators..” Has this been proven? Do most predators approach from outside or within the canopy? I think this is largely speculation, unless you can back the statement up with some good references.

14. p. 19 lines 4-6 from bottom. This statement needs some work. The effects of edge on predation risk are far from clear, and likely depend on the type of edge involved. The Nelson & Hamer and Manley & Nelson papers both used strongly overlapping data bases, and do indeed show negative edge effects. The work by Bradley (reviewed in McShane et al. 2004) showed different patterns, and cannot

be ignored here. You do discuss this work later (p. 28) but it should also be mentioned here.

15. p. 19 last line. “high tree density”. This is not correct. Many studies have shown negative associations between occupancy or nests and tree density. The statement also contradicts the results shown in Chapter 4 p. 3 of this report. Better so say density of large trees, or high basal area resulting from large trees.

16. p. 20. The review does not mention any of the landscape-level habitat associations revealed by radar studies (e.g., Burger 2001, Raphael et al. 2002).

**Reply:** Cited Burger 2002 instead of Burger 2001....more recent information. Raphael et al. 2002a is cited in this discussion.

17. p. 22 last para. The first part of this paragraph needs some references to make it more credible.

18. p. 24 para 2. This subheading Slope and aspect is a bit misleading because there are other parameters discussed here too – moisture and distance inland. Last few lines on this page: Surely distance inland per se is important here – at some point the birds are not going to use distant habitat even though there might be suitable platforms. If correct, might be better to add “At distant sites well within the expected flight range of nesting murrelets .....

19. p. 25 middle. These statements apply to Desolation Sound. There were a few nests on steep slopes in Clayoquot Sound, but no evidence that slope was preferred.

The next sentence cites Manley (1999) as “other areas” but her study was also done in the Desolation Sound area.

20. p. 26. The effect of distance from the coast is not a simple linear relationship with preferences for being near the shore. You discuss the avoidance of shoreline habitat in the next paragraph, but there is obviously a non-linear distribution within the next 30 km (see for example Hamer and Nelson 1995 Fig 1). The relationship depends partially on the scale at which the analysis was done. Meyer’s study was done at relatively large landscape scales. Might be more accurate to state that effects of distance from shore are likely to become important as the commuting distance reaches some threshold.

**Reply:** The relationship was not stated as linear. Factors are state that could affect distance from the coast. We know nothing about commuting distance thresholds (other than the 55+ miles birds fly inland).

The first line of the next paragraph sounds like the birds do not often nest on the water! I think you mean in shoreline forests.

21. p. 27 middle. The paper by Cam et al. (2003) is relevant to these topics and could replace Bradley et al.

**Reply:** Included both.

Near the end of this paragraph – explain to non-ornithological readers what a corvid is (ravens, crows and jays).

22. p. 28 middle. Ratti and Reese (1988) as a general reference for predation at edges is hopelessly outdated. There have been hundreds of edge studies since then and some good reviews, for example:

Chalfoun, A.D., F.R. Thompson, and M.J. Ratnaswamy. 2002. Nest predators and fragmentation: a review and meta-analysis. *Conservation Biology* 16:306–318.

Haskell, D.G. 1995. A reevaluation of the effects of forest fragmentation on rates of bird-nest predation. *Conservation Biology* 9:1316–1318.

Marzluff, J.M., and M. Restani. 1999. The effects of forest fragmentation on avian nest predation. Pages 155–169 in J.L. Rochelle, L.A. Lehmann, and J. Wisniewski, editors. *Forest fragmentation: wildlife and management implications*. Brill, Leiden.

Paton, P.W.C. 1994. The effect of an edge on avian nest success: how strong is the evidence? *Conservation Biology* 8:17–26.

**Reply:** Did not change reference. Ratti and Reese have done the best study on the effect of types of edge on predation. None of the papers listed here address hard and soft edges.

23. p. 28 bottom. Perhaps reiterate the information given on p. 21 that old-growth buffered by larger second-growth forests showed reduced predation at artificial murrelet nests.

24. p. 29 line 6-7. “some distance inland from the open ocean” is irrelevant. The birds were feeding within Desolation Sound itself and in the Strait of Georgia. Better to focus on the distance from nests to marine foraging areas and not open ocean.

25. p. 30 line 5. Better to say “through the water using their wings, in pursuit ...”

26. p. 31 para 2 and in the reference list. Change Bakum to Bakun.

27. p. 31 line 4 from bottom. Better to say “Within this southern area, murrelet distribution ....” because the preceding sentence mentions northern areas as well so it isn’t clear what “this area” refers to.

28. p. 33 last para. It is misleading to simply give a range of commuting distances, because very few murrelets flew 1 or 120 miles. Better to give some

indication of what the general central tendency is. Hull et al. (2001) is relevant here too.

**Reply:** Added reference, but did not include a general central tendency for commuting distance because this is not known.

29. p. 34 line 6. Give the years over which Strong (2003) showed a decline. The statements about Clayoquot and Barkley Sound are not really correct. It was not the total population – just the birds counted in grid surveys. There were complicating effects of warm oceans (see Burger 2002 review and Burger 2000).

30. p. 34 end of main paragraph. “There are no data ...” One weakness of this entire NWFP report is that it ignores other studies (albeit less rigorous) that might indicate population trends. For example, at-sea counts in Puget Sound seem to show rather alarming declines (recent paper by Bower et al. at the 2005 PSG conference in Portland) but are not mentioned here. Surely there are many other similar data that could be reviewed for crude indications of change?

31. p. 35 line 9 – should read “carrying capacity”

32. Literature Cited

There is inconsistency in when [et al.] is added to the author list. It seems redundant to have it there. Even non-scientists should figure out what et al. means.

**Reply:** Just following the PNW guidelines.

Species names are not always italicized.

This paper cited in the text is missing from the lit. cited list:

- Strong (2003)

p. 38 Bradley is mis-spelled in Bradley et al. 2002.

p. 46. The material in the technical report by Lougheed has been published and the journal papers make a better citation.

Lougheed, C., B. A. Vanderkist, L. W. Lougheed, and F. Cooke. 2002.

Techniques for investigating breeding chronology in Marbled Murrelets, Desolation Sound, British Columbia. Condor 104:319-330.

Lougheed, C., L. W. Lougheed, F. Cooke, and S. Boyd. 2002. Local survival of adult and juvenile Marbled Murrelets and their importance for estimating reproductive success. Condor 104:309-318.

**Reply:** Added one of these references to paper but kept Lougheed 2000 as the 2002 papers do not address marine habitat issues.



Same page – indicate that Manley 1999 is an MSc thesis.

Check Waterhouse et al. 2002. It is cited as 2004 in one place in the text.

On p. 8 line 9 indicate whether McFarlane Tranquilla 2003 is a or b.

### **Reviewer #3**

p. 4: specify the geographic domain of the statement that 50-90% of old growth habitat suitable for murrelets has been lost – range wide? plan wide? zone wide?

p. 9: specify the geographic domain of the timing of breeding statement e.g. “Within the plan area...” Is there a temporal gradient among zones?

p. 21: statement about Raphael et al. finding higher occupation with more edge seems to contradict topic sentence of the paragraph.

p. 23-4: statement about use of higher elevations being related to loss of lower elevations is a repeat of speculation from the sources cited. It is at least equally likely that higher elevation sites were used even when lower elevation sites were entirely available. In some landscape, higher elevation sites may in fact be better (safer) habitat than lowland areas.

citations:

p.23,24,25,29: Huettmann et al. 2003 should be cited as an MS, or in review, or personal communication.

p.27,40: Cam et al in press has been published: Cam, E., L. Loughheed, R. Bradley and F. Cooke. 2003. Demographic assessment of a Marbled Murrelet population from capture-mark-recapture and radio telemetry data. *Conservation Biology*:17:1118-1126.

p. 27,38: Bradley et al. in review has been published: Bradley, R.W., F. Cooke, L.W. Loughheed, W. S. Boyd. 2004. Inferring breeding success through radio telemetry in the Marbled Murrelet. *J. Wildl. Manage.* 68: 318-331.

## **Chapter 3**

### **Reviewer #1**

**Comment:** “Only part of objective 2 in the Marbled Murrelet Effectiveness Monitoring Plan outlined in Madsen et al. (1999) was addressed because reproductive rates of the marbled murrelet were not assessed in the report and results for just 4 years of monitoring were given, rather than 10 years.”

**REPLY:** We have included some discussion in the chapter as to why productivity indices were not addressed in the population monitoring and results for of monitoring are for 4 years.

**Comment:** “The at-sea monitoring survey design was changed to create consistency across the plan area. This was needed, but it created a disconnect with data collected previous to 2000. When the switch was made, more data should have been collected to create a crosswalk between the old monitoring methods in each area and the new monitoring survey design. With such a crosswalk, population trends could be estimated over 10 years, rather than 4.”

**REPLY:** Some discussion has been added to the Chapter introduction and Discussion on this topic.

**Comment:** The conclusions for the population size and trend of murrelets at sea are appropriate for 2000-2003. The authors correctly concluded that more surveys are needed to detect a change, but perhaps more conclusions could be drawn by examining data prior to 2000 after the appropriate crosswalks are developed to make the data comparable. “The authors also concluded that the murrelets in the plan area are only a small proportion of the total murrelet population”

**REPLY:** The meaning of the text was unclear and has been edited.

**Comment:** My main question for this chapter is why it ignores assessing population trends from data prior to the last four years. I recognize that the authors are concerned that the new survey design does not allow comparison to data from prior surveys that used different methods. However, data within states was often collected by one set of investigators using consistent methods in the 1990s, and trends during that time period in that data should be presented and discussed in this chapter. Furthermore, it is possible to crosswalk the results from the last 4-years to previous survey data by computer simulations. In Washington, apparently surveys were conducted during similar time periods using the different methods to allow a crosswalk. I suggest that crosswalk be a high priority for funding in the future. Comments on this general topic later in this document. Finally, no information is given on juvenile: adult ratios as an assessment of productivity. Why not? The study says birds were counted. Do the counts include juveniles?

**REPLY:** Similar issues to the two first comments. They have been addressed.

### **Page Specific Comments:**

p. 7. Selecting PSUs for sampling--second paragraph. Discuss the possible disadvantages of resampling the same PSUs.

**REPLY:** The samples were randomly selected, even for resampling. There is no disadvantage.

p. 8. Bottom of page--r is defined mathematically, but not in the text.

**REPLY:** Text has been edited.

p. 14. First full paragraph. Describe how pairing of the inshore and offshore PSU subunits was accounted for in the estimation of density and variance.

**REPLY:** This paragraph was no longer relevant and was deleted.

p. 14 Zone and Population Level Estimates--This paragraph is unclear as to what the model-assisted approach was (no citation given). It is hard to believe that the data did not show temporal trends, given that fewer adults are probably on the water early in the season during the incubation phase, and that juveniles become more abundant toward the end of the sampling period.

**REPLY:** This paragraph was deleted. We had rejected the model-assisted approach to address potential temporal and spatial effects on counts as we found no effect. The paragraph added confusion. Discussion was added regarding lack of temporal effects on counts.

p. 15. Last sentence. It says "we calculated an estimate of the standard error". Is this the standard error of the zone estimates?

**REPLY:** The text was edited to indicate the standard error is for all zones combined.

p. 16. Estimates of Target Population Change, 2<sup>nd</sup> sentence--It says "Because the annual means and variances were approximately equal..". I don't think including means in that sentence is correct for regression assumptions because the purpose of the regression is to test if the means differ.

**REPLY:** Means was deleted.

## **Reviewer #2**

**Comment:** This chapter reflects the huge efforts that have been made in the NWFP to standardize at-sea survey methods, and the analysis using these surveys to estimate murrelet populations. Whether at-sea surveys are the most reliable or powerful method to track murrelet populations depends on the size of the population and inland topography (radar surveys are probably better in most areas, especially where topography restricts flight paths). Nevertheless this paper represents the best effort to date in using at-sea surveys to estimate total populations for the 3-state area. The results seem to provide relatively consistent population estimates, but do not cover sufficient years to detect any population trends.

**REPLY:** None necessary.

**Comment:** The discussion in this chapter is disappointing and inadequate. There are several points that could be discussed in more detail to make the chapter more informative and provide a better sense of the strengths and limitations of the data and the methods. There is a lot of emphasis on the survey methods and statistical approach, but insufficient emphasis on the implications of the results.

**REPLY:** The implication of results is being presented in the synthesis sections of the 10-yr report.

**Comment:** The biggest problem is the failure to make any comparisons with previous population estimates from the 3 states. The many previous population estimates are cited (p. 24 para 1) but then dismissed because they are “not directly comparable”. Some explanation is needed for this dismissal. Even if the methods and analysis were different I think most readers would like to see a comparison and get some sense of what the populations are doing beyond the 4 years of this study. Are the new estimates higher or lower than previous estimates? At least provide a table which summarizes previous estimates and compares them with these new estimates. If confidence limits are given, this will give some idea of the precision of each study, even if the accuracy (real population size) might be more difficult to assess. Perhaps indicate on the table the shortcomings of the previous estimates, but at least allow readers to see what other studies had found.

**REPLY:** We do not have this information for all states or zones. We do have -- Calif.: Ralph and Miller, Cons. Assess. (1989-1993 data); Miller et al., Waterbirds (1989-1998 data), Oregon: Strong et al. Cons. Assess. (1992-1993 data); Strong, 2003; Oregon and Wash: Varoujean and Williams Cons. Assess. (August and September single aerial surveys 1993); Washington: Speich and Wahl Cons. Assess. (1971-1993 data various locations and methods); Ralph et al. 1994 and 1996; Raphael et al. 1996. Various statistical considerations also preclude our full assessment of the pre-2000 data. The table with prior population estimates is being added to Chapter 2. Some text on differences in methods has been added to discussion.

**Comment:** The “difference in methods” argument does not hold for the recent studies in California by Peery and others in Beissinger’s group. They too used Distance sampling along line transects. How do their published population estimates compare with those in this study for the same areas?

**REPLY:** There are no overlapping areas with the area of their study, Zone 6.

**Comment:** The chapter provides a fairly rigorous analysis of the power to detect trends if years of effort are varied (e.g. p. 22 and 23 and Table 9), but this doesn’t readily address the issue of reduced survey effort within years or within specific

zones. Is it realistic to expect the same survey effort within each year to continue for the next 6-15 years? Some information on the likely effects of reduced survey effort would be useful, plus recommendations, based on this analysis, on which coastal zones should have priority with reduced funding?

**REPLY:** It is likely the standard error of the estimates will go up if sample size is reduced. Reduced effort in Zone 4 and Zone 5 in 2004 due to a change in funding levels may provide an example of this when results are presented. The sampling design assures statistical validity even with reduced samples.

### **Specific comments on Chapter 3:**

1. The chapter lacks an Abstract – the other chapters have one.

**REPLY:** An abstract has been added.

2. p. 2 top paragraph. Will continued at-sea and inland habitat surveys show the relationship between habitat availability and population trend with sufficient resolution and power? It seems the best analysis possible would show the relatively crude relationships between estimated numbers and estimated habitat areas seen in Chapter 5 (Fig. 14). Radar counts at the watershed level seem a better way to establish these relationships

**REPLY:** Additional text had been added to the introduction addressing our consideration of radar.

3. p. 2 para 2. “general consensus ....” and then later “Population estimation is not feasible at inland nesting areas”. These statements are not true. In BC the governments and recovery team have decided to use radar counts to monitor population trends, not at-sea surveys. Radar counts have high power to detect trends (recent studies by Arcese et al. in BC and Bigger et al. in California). Radar *can* accurately detect and monitor populations in inland areas. It seems the main value for continued at-sea surveys is the historical value given the absence of many past radar counts and the difficulties of counting murrelets with radar at low-lying open coastlines.

**REPLY:** Our target populations are at the Zone level, while in BC and Bigger are using them for the much smaller watershed level. Text has been added. See number 2.

4. p. 4 para 2. Huff et al. (this volume) does not show the six conservation zones (obviously an oversight because there is no map within the entire report showing all zones).

**REPLY:** Zone 6 is not within the Forest Plan area.

Same paragraph few lines down: some very vague statements about “few murrelets, ... very small proportion ...” can you give a rough idea of what % of the overall count was far offshore?

**REPLY:** The percent was added to the discussion.

5. p. 5 end of para 1. The proportions that breed that are given here are misleading, or at least only apply to the southern part of the species range. Sealy (1975) showed that 85% of the birds he shot had enlarged brood patches and gonads. McFarlane Tranquilla et al. (2001) showed 46-80% (mean 55%) of females with vitellogenin. Bradley (2002) suggested that a minimum of 62.5% of radio-tagged birds showed breeding activity. These are in reviews by Burger (2002) and McShane et al. (2004).

**REPLY:** The statement has been updated.

6. p. 5 last sentence. What was the separate population estimate? Is there a good reference for this?

**REPLY:** Reference to tables 3-6 was included.

7. p. 9 middle paragraph. This section glosses over the effects of the boat on murrelets. There is no doubt that murrelets move away from approaching boats and might take flight or dive. What effect does this have on the population estimates? You might be traveling slow enough to see a murrelet re-surface if it dives (but I have doubts about this too – see below), but it will almost certainly not be in the same place if the boat is nearby. You need to discuss the effects of birds moving away from the mid-line on the detection curve what effect this might ultimately have on the population estimate. Note that Strong et al. (1995: Fig. 1) showed fewer murrelets along the mid-line than expected.

Potential sources of error (and their effects on the resulting population estimates) are briefly mentioned in the methods, but need closer scrutiny (see point 7).

**REPLY:** Observers are trained to use scanning patterns to minimize missing birds on the transect line. Distance estimates are made at the moment the birds is seen and scanning continues while the detection is recorded.

A boat traveling 15 knots (7.7 m/s) will cover about 230 m during a 30 s dive by a murrelet (moderate dive time) – I’m not convinced you aren’t missing many birds that dive, particularly if there are many birds about and the observers are busy estimating distances. This needs to be discussed, ideally using examples such as this.

**REPLY:** A boat would only travel at 15 knots in very calm water, when murrelets can be seen well beyond 100 m from the boat. The birds tend not to dive in response to the boats at that distance. If they do dive, there is a good chance to see them when they resurface.

I'm convinced you have done a good job in reducing potential errors with the Distance method but at least mention these unavoidable sources of error.

**REPLY:** These have been included in the text.

The last sentence in this paragraph mentions studying the effects of the assumptions on the estimates but we need see some indication here what these effects are likely to be – are you likely to be underestimating or overestimating the populations?

**REPLY:** Text has been edited.

Another potential source of error mentioned on the next page but not adequately discussed arises from using one central distance measure for a group of murrelets.

**REPLY:** Average group sizes,  $E(s)$ , are less than 2 birds. By definition a group of birds must be less than 1.5 m apart to be considered in one group. Any error from using the center of the group as a distance would be extremely small. In addition, we use the DISTANCE adjustments for the effects of group size on detectability.

8. p. 19 Results first para line 4. Is the total population area mentioned here the marine area for all zones?

**REPLY:** Yes, our target population is on the water and the text so indicates.

9. p. 19 last 3 lines – this is repeating what has been said in the preceding paragraph and introduction.

**REPLY:** Text was deleted.

10. p. 20. para 2 last line. How do these estimates compare with those of Peery et al. (2004 Condor and other papers)?

**REPLY:** Peery et al. estimates are for Zone 6 only. A review of past estimates has been added to Chapter 2.

11. p. 21 para 2 last 2 sentences. Figure 9 still shows a data point for 2000 for Zone 2. I assume from this text that it should not be there.

**REPLY:** The points have been removed from the figures.

12. p. 22 line 7. I think you mean Tables 9a and 9b.

**REPLY:** Yes, text has been corrected.

13. p. 23 para 1. Does the flexibility mentioned to here refer to the data on varying years of survey (on the previous page)? – perhaps make this more explicit.

**REPLY:** Edited to clarify.

14. Table 1. The wording of the “area of inference” suggests that the extrapolation of the counts did not include areas within 300 or 350 m of the shore. There is nothing on this in the methods section. If this interpretation is incorrect then the wording needs to be more clear. If this is correct then all murrelets closer than 300-350 m would appear to be ignored, which could be a substantial portion of the population. In many areas within the 3-state region high concentrations were found close to shore (e.g., Strong et al. 1995 Fig. 5&6; Speich & Wahl 1995) and this is true for BC and AK too (Burger 1995, Day et al. 2003, Day & Nigro 2000 – Fig. 7), but perhaps not CA (Ralph & Miller 1995 – Fig.3). Presumably this issue was considered in establishing the survey design – explain the choice of this boundary and what the implications might be for underestimating populations.

**REPLY:** Additional detail was added to the paragraph defining the target population. Our inshore boundaries include the “close to shore” distances the reviewer refers to where high concentrations of murrelets have been seen. Past observed murrelet distributions were considered in the sampling design.

15. Tables 3-6. Perhaps remind readers what  $f(0)$ ,  $E(s)$  etc. stand for. Tables should be relatively self-explanatory.

**REPLY:** Added to table captions.

16. Table 8 should be deleted. What is the point of showing estimated % change when none of the regression lines was significantly different from zero (Table 7)? There is a real risk that the data in Table 8 will be misquoted by those too lazy to read the whole paper, and some people will assume that the populations are increasing in zones 1-3. Why does Zone 3 show a positive change in this table but a negative trend in Figs 8 & 9?

**REPLY:** Zone 3 in Table 8 has been corrected. The authors preferred to include this table to provide values for the confidence intervals.

17. Figure 1 and 2. Which stratum did the west coast of Whidby Island fall into – not clear from maps.

**REPLY:** West. The maps will be updated.



18. Literature Cited. The citation of references is full of errors.

**REPLY:** Literature cited has been updated.

### **Reviewer #3**

***1. The overall conceptual design of the monitoring program. Is it well conceived? Are there any major flaws?***

This program had the ambitious goal of monitoring population distribution, size, and trend over nearly 9,000 km<sup>2</sup> of ocean. Given sufficient resources, the design is fundamentally capable of doing so.

**Comment:** The introduction states that the eventual goal of the program is to “infer population trends based on the amount and distribution of nesting habitat and cease monitoring offshore murrelet populations themselves”. Taken literally, this approach would indeed be flawed, since oceanographic changes, or changes in predator populations, could alter murrelet populations independently of nesting habitat availability. The authors recognize and qualify this potential inference in the immediately following sentence (p.2: “...and incorporates additional factors affecting murrelet abundance”, but I suggest they reword the first of these statements. In a world of multiple limiting factors, theoretically, murrelets could disappear locally independently of suitable forest habitat availability.

**REPLY:** The reviewer has made good points here and the text has been edited.

**Comment:** The general Abstract for this publication correctly specifies that the value being estimated in this monitoring is the “population size at sea ... (on any single day)” (p. vii) for the area in question. Within Chapter 3, the authors need to maintain focus on the meaning of this variable in two respects. The first is the “at-sea” component. These counts will under-represent local population sizes by not including birds incubating during the study period, which represent perhaps 10-30% of local populations during the times sampled. The preceding rough range calculation assumes that one member of each breeding pair would be incubating during surveys for 35 days of the survey period of ca. 85 days (including some allowance for renesting by some pairs when first nests fail), that adults represent ca. 85% of populations and that ca. 50% of adults breed at some point during the season (this maybe high or low) - these estimates suggest that 17.5% of murrelets were incubating when sampling occurred. Although it would not be precise, and is not directly relevant to the question of monitoring a trend, it would nonetheless be informative for the authors to provide some calculation along the lines I suggest above, despite the uncertainty involved.

People want to know “How many murrelets are out there during the breeding season”, and this answer should consider all birds. People will cite the surveys totals as “Population Estimates” for zones or for the plan area as a whole.

Specific example where overlooking this becomes a problem:

p. 23 “We acknowledge that our target population estimates does not include the entire murrelet population in our zones. ....because the surveys do not include further offshore sites.” But with respect to the total population, they also miss the birds incubating in the woods, despite falling within the definition of the broader “target population” within the plan.

**REPLY:** Text has been added to clarify for the readers that we are estimating the average number of birds per day in our target area and target season.

**Comment:** These are minor comments. For purposes of estimating population status and trends along these coastlines and relating them to other factors, including spatial and temporal changes in forest nesting habitat availability, the basic approach is sound. It is not obvious that any other approach would be more practicable or appropriate.

## **2. *The analytical approaches adopted. Are they well justified and using appropriate methodology?***

**Comment:** Given the long-term nature of this monitoring program, taking time to field test and simulate sampling schemes will prove to have been necessary and warranted in the long run. It is nonetheless unfortunate that a standardized sampling could not have been put in place prior to 2000.

**REPLY:** 2000 was the target date presented in the Marbled Murrelet EM Plan to begin surveys.

**Comment:** PSU definition: as outlined under heading 1, the fundamental sampling unit is really a transect within a PSU on one day. Discussing random selection of PSUs is a bit misleading, since (1) nearly all (e.g. 160/167) were sampled each year and some were even resampled in certain zones, and (2) the authors ensured that units sampled in 2000 were sampled in subsequent years. This is so close to being comprehensive that nearly all of the “sampling” is really occurring at the “subunit” segment transect level (see further comments about variance estimation below).

**REPLY:** Text has been edited to clarify.

**Comment:** Offshore sub-unit sampling approach:

The methods refer to paired on-shore and offshore sampling within PSUs. The offshore unit covers “a portion of the PSU’s length, or in some cases the entire length”. In Figure 7, there appears to be no outer zone zigzag transect paired

with the inshore transects for Segment C, consistent with “a portion of the PSU’s length” being sampled. Does this explain this omission.

**REPLY:** Text has been edited to correct explanation.

**Comment:** It would be helpful to have some explanation of the procedure or protocols for determining specifically where outer transects would be more or less completely sampled.

**REPLY:** For some zones the length of the offshore zigzag transect does not sample the entire length of the PSU each survey day. The start point of the transect is randomly selected for each survey, therefore, the probability of being sampled is the same throughout the subunit.

**Comment:** I note that using Cochran’s formula for optimal allocation of effort between subunit types seems completely appropriate.

p. 13: I assume that the 1000 in the formula represents a meter to km conversion somewhere?

**REPLY:** Yes and added to text.

**Comment:** It is unclear from the information presented whether the bootstrapping designed to deal with potential autocorrelated clustering effects (p. 13), and the elaborate PSU variance estimation procedures, including those designed to deal with pairing of inshore and offshore PSUs (p14) are in fact necessary. It may be conservative and appropriate to use them. However, the transect data themselves are fixed, and it is not clear that the basic transect variance itself is captured by the methods used. It is possible that a more straightforward procedure, taken as variance among the PSU segment transects estimate needed to form an overall estimate, would be sufficient. My uncertainty about the need for the procedures described might be dealt with in the more detailed document Raphael et al. MS referred to in this chapter. Another way to put my concern is to ask whether or not one can separate process and sampling errors in these estimates?

**REPLY:** We addressed the issue of variance in counts at different scales when we examined the past data. The PSU size selected was based partially on that examination and on the constraint of what transect length could be regularly accomplished in a day. There is considerable variance in the day to day counts at the PSU scale and we selected this method of analysis to best measure it.

**Comment:** Approximately 3 MS pages (17-19) on power analysis methodology could be shortened or eliminated by simply referencing Gerrodette 1987 (Ecology 68: 1364-1372) for power analysis formulas and approach. Even if the authors want to be maintain their explicit presentation, formula references from that paper

could be used. For clarity, in the last line on p.17, “individual observations” should I think be “observations from individual years”

**REPLY:** Clarified in text.

**Comment:** Additional null hypotheses of population change (increases, “change in either direction”) could obviously be tested with the data collected, but the authors have probably been wise in restricting the current analysis to calculation of power to detect decreases.

**REPLY:** We agree.

**Comment:** Please specify how specific dates of sampling were chosen, or provide some idea of how such decision was or was not constrained by the logistical complications one might expect when trying to execute such an extensive and complex operation. The statement in Table 1 re: PSU selection is just too vague. Any reviewer recognizes that practical considerations do not allow for perfect random sampling over time and space, and the authors have in fact incorporated clustering of sampling into their estimation of variance. All that is provided is the statement on p. 5 that surveys were to run between 15 May and July, ending earlier in Zone 5, and a statement that no temporal “trends” were detected in the 2000 data (p.14). Some more detail regarding the temporal distribution of effort is in order.

**REPLY:** Text has been edited to clarify.

**Comment:** A second reason to keep in mind that the fundamental quantity being estimated is “at sea murrelets on one day” (see comments under #1, above) relates to potential seasonal effects due to date. A specific example where this has not been kept in mind: p. 11, bottom: “We estimated the average total number of birds” should be “average daily total numbers of birds”. More generally, although the sampling period has been windowed within a range of dates, and the authors have not detected date effects in 2000 (p. 14), they almost certainly exist and will become manifest as additional data are added to the data set. It is not clear whether linear or U-shaped date effects were tested for. Eventually, it seems likely that the precision of estimates will be improved by taking date into consideration in some fashion. A linear correction may not be appropriate, but lower values on the both the leading and trailing edges are plausible and could be corrected for eventually, improving power by reducing within year variance.

**REPLY:** Text edited to clarify.

**3.     *The data reported. To the extent that you can tell from the mss, are these well developed, and collected using appropriate techniques?***

The Distance Sampling is entirely appropriate and has been carried out with well trained personnel. I have discussed sampling and analytical considerations above.

**Comment:** p. 21: second paragraph: rather than “Our results indicate that the size of the target population did not change significantly”, it would be better to state “Our results provide no evidence of a change in target population size”.

**REPLY:** Changed.

**Comment:** As per my comments about what is being estimated under points 1 and 2, the headings for Tables 3-6 should say “target population size”, as should Figs 8 and 10.

**REPLY:** Changed.

Minor comments:

Third paragraph: “widest CI” rather than “largest CI” -- Edited.

Table 7: p values show way too many decimal places.

Tables 8: The estimate of annual percent change for zone 3 is missing a minus sign.

**REPLY:** Above edits completed.

Fig. 9 might be eliminated, or perhaps replaced with a Figure for the entire Plan area only. Showing non-significant fitted lines with no CIs is misleading, and persons wishing to examine trends within zones visually can do so from Fig. 8.

**REPLY:** They will be added later.

p. 22: last sentence of Results: “changes of 10% annually in less than 20 years”.

**REPLY:** Corrected.

**4. *The conclusions. Are these appropriate given the results reported and the limitations and constraints inherent in the monitoring design and data? Should the conclusions be stronger/less strong?***

**Comment:** The authors state (p. 4) that “Considering that we present results from only 4 years of surveys, information in this report on the status and trend of the Marbled Murrelet is preliminary in nature”. This is only half correct; the authors sell the program short. While examination of a trend is indeed premature, these data do present a coherent “status report” for the period covered. These data will provide a reliable benchmark and starting point against which future survey data may be compared with confidence.

**REPLY:** The reviewer’s suggestion was incorporated into the text.

**Comment:** I agree that the estimates for target population sizes derived in this study should not, in most cases, be compared with earlier ones derived with quite different methods. Certain local comparisons, however, might be valid.

**REPLY:** Authors prefer to make comparisons once the crosswalk is attempted.

**Comment:** Note the comments about “at sea” vs total local population size made under point 1 (p.23).

**REPLY:** Noted and changed.

**5. *Are there changes to the program that should be considered? Is the program adequate to the task of providing effective monitoring for the Plan under the objectives set out?***

**Comments:** I believe that this program would detect real trends in local population size of the magnitudes considered for zones 1-4. One must keep in mind the several points, however. First, real changes in local population sizes are likely to lag decreases in habitat by 5 or 10 years due to local philopatry by adult breeders. Adjustments to forest management would further lag any findings. It is thus important to maintain a regular monitoring schedule.

That being said, now that four years of solid and consistent data provide a good basis for estimating the real within-year variances involved, on both zone and NWF planwise level, the authors should consider whether annual censuses still make sense. The rates of change in forest availability appear to have themselves declined, and biennial or triennial surveys might be a reasonable future protocol with sufficient power to demonstrate trends over longer periods of time in a still timely fashion. A strategy involving punctuated surveying might not be sensible until oceanographic correlates of variation are better understood. In the absence of such knowledge, interannual variation (as opposed to trends attributable to changes in nesting habitat) is more likely to swamp trend detection with less than annual sampling. Nonetheless, those responsible for maintaining monitoring should simulate the magnitude of loss of information from doing so and assess the situation. I recognize that difficulties in this approach include the annual nature of budget lines and continuity of trained observer and supervisory personnel. I would not recommend a system involving rotating surveys (e.g. 2 or 3 zones per year). Birds may shift distributions with oceanographic conditions, and only comprehensive surveys will enable such annual shifts to be detected.

I also would not generally recommend surveys with substantially less effort (e.g. fewer PSU transect days). However, a second potential way to economize without losing continuity with existing information would be to further decrease sampling in the outer stratum, which contributes little to the total power for trend

estimation. A shift towards foraging further from the coast, which comparison of the outer and inner stratum might detect, might also be detectable as shifts in distribution between transects of the inner stratum itself. within the transects within would have to be made Although retaining these surveys would enable detection of a shift towards further offshore foraging,

The authors have reduced their sampling in zone 5, where this approach is less powerful. It may be more effective to concentrate sampling in a finer-grained way within this zone, even if this means future inconsistency. As the authors admit, this broad-scale survey approach is not nearly as effective within that zone.

Since the surveys did not begin until 2000, we do not have a 10-year benchmark for effectiveness monitoring as originally envisioned. I recommend that a formal summary document be produced 5 years hence (e.g. 2009-2010) to reevaluate the results, rather than waiting for another 10 years to do so, if this is the current expectation.

**REPLY:** Not necessary for comments in this section

## Chapter 5

### Reviewer #1

In the 10-year monitoring effectiveness monitoring report, objective 1 was addressed, except the models developed for predicting nesting habitat were mostly based on the stand scale, not the landscape scale.

**REPLY:** We have addressed this comment by adding landscape scale variables into our analysis.

One flaw in the conceptual design is that the focus in the 10-year report on nesting habitat ignores the important role of the marine environment. In the monitoring plan, a conceptual model (Figure 2) is provided that includes marine factors, but no variables of proximity to areas of high marine quality have been included any of the nesting habitat models of the 10-year report.

**REPLY:** We agree that marine factors can be important, but a decision was made early on to focus on inland habitat, as that is the under the control of the NWFP whereas marine factors are not. We have mentioned the importance of marine factors in several places in the chapter.

Going beyond the vision of the current monitoring plan, I believe that the murrelet habitat in the Northwest Forest plan area needs to be understood in the context of the range of the entire species, rather than in isolation. This is especially important because the species might be considered for delisting on the basis on population estimates, genetics, and habitat available in areas north of the plan area. The regressions models developed that relate inland habitat to offshore murrelet population size might be much stronger if they encompassed a wider range of conditions. Nests, occupied/unoccupied sites, offshore density or radar could be used to develop such range-wide habitat models.

Once a predictive range-wide habitat model is developed, a spatially-explicit metapopulation model that covers the entire geographic range of the marbled murrelet and incorporates genetic information flow could be developed to assess extinction probabilities under different timber management scenarios in combination with global warming in the Northwest Plan Area. Both modeling efforts would require extensive collaboration of murrelet researchers, geneticists, and modelers. Nevertheless, I believe it is possible and recommend that such an approach be incorporated into the next 10-year monitoring cycle.

**REPLY:** This is an excellent suggestion, and an effort is underway to develop a rangewide model. Clearly, such a model is beyond the scope of our current NWFP effort.

This chapter was well-written, used some interesting new approaches, and had much potential. I liked the use of remotely sensed variables that included all of the lands in the plan area, rather than just the lands with inventory plots. It also evaluated changes in habitat over time and compared inland habitat quantity with offshore population estimates, which is greatly needed. I also like that it built on information gained from Chapter 4 when choosing variables for the ENFA model and that their approach tested if a simple expert model would do the job. Unfortunately, when I saw the maps of the suitable habitat developed from the models, I was disappointed with the expert judgment models. The estimates from the expert models overestimated habitat in California and Oregon in the areas I consider to be outside the nesting range even in zone 1 (that have been heavily surveyed and have practically no occupied behaviors). The ENFA model appeared to do a much better job, although it probably overestimated habitat in the inland portion of Oregon in Zone 1 (mainly when using >60 as the cutoff), and underestimated it in California.

**REPLY:** Our revised models seem to have achieved what the reviewer felt were better distributions in California and Oregon – amounts of habitat in each area moved in the direction recommended by the reviewer.

If folks disagree, then I would like to see more surveys conducted in such areas considered to be potential habitat to validate these maps. I think it was appropriate to conduct separate ENFA models on each state, but that also resulted in a small sample size in California, making that a weaker model. Also, one model on the entire area might be possible if variables that quantify important differences among the states are included in the model (e.g., climate, dominant tree species, etc.).



**REPLY:** The BioMapper software can not be run for the entire range due to memory constraints. However, we did merge the Oregon Klamath with California in the revised models, and this increased the sample size substantially.

I would have liked to have seen use of landscape pattern variables, other than patch size >1000 (which was not even used to predict habitat suitability). The marbled murrelet effectiveness monitoring plan (Madsen et al. 1999) discusses the importance of assessing the contiguity and fragmentation of nesting habitat (patch size, spacing between patches, connectivity, etc.), but this was not addressed using model variables or adequately in the entire 10-year report.

**REPLY:** We added two measures of landscape pattern (patch area and patch proximity) to the ENFA models and reran the entire analysis. These variables were important in the models, and resulted in a different configuration of suitable habitat. We believe this was a significant improvement in the models and thank the reviewer for the suggestion.

Moreover, the plan mentions comparing offshore murrelet densities or productivity (juvenile to adult ratios) to large blocks ("landscape units" of 31-62 miles south to north) of inland habitat, yet this was not done. Instead, it was crudely done for offshore densities using the 5 much larger conservation zones (only 5 data points in Fig. 14a and 14b). I say crudely because the inland habitat estimates did not account for connectivity of the habitat or a time lag of offshore populations in response to habitat changes and fragmentation, which was found to be important in California and southern Oregon (Miller et al. 2002). Perhaps a more refined analysis could be added in the future.

**REPLY:** We subdivided the analysis into smaller strata for a more refined analysis. It is still fairly crude, but a significant improvement over the original approach.

I don't feel that the nesting habitat models are accurate enough yet to use for large-scale management decisions (e.g., can't use the models to alter the pattern of the harvest regime in large areas and see what happens), and they are not good enough to substitute for offshore-monitoring (a goal described in Madsen et al. 1999). More refined models developed in the next monitoring cycle using remotely-sensed information that incorporate habitat fragmentation, time lags, and marine habitat possibly could be useful.

#### Introduction:

It would be nice to relate the introduction of this chapter to the previous chapter, emphasizing what new information is gained using the approaches outlined in chapter 5 over what was learned in chapter 4. Based on the question on page 7, one expects an assessment of interior habitat (core area) in the chapter, but it did not happen. It should be made clear in the introduction why landscape pattern metrics were not included in habitat suitability models, even though such metrics have been very predictive of murrelet use of areas in other studies. If small, isolated patches (< 4 to 10 ha) are not likely to be used, is that important to assess at this scale? Also, mention that patch size (> 1000 acres) distribution was assessed descriptively, but was not a variable in the models.

Later in the chapter on p. 20, it says "regardless of size". That is confusing because what that means has not been explained up front in the introduction.

**REPLY:** As noted above, we have rerun the models with landscape variables, so this comment is now reflected in the final draft.

Methods: (I realize methods, results, and discussion are not headings in this chapter, but I have organized my comments by these more general categories)

p. 10. Last sentence of first partial paragraph. The national and state lands within the Redwood National Park boundary in California are jointly managed by the National Park Service and California State Park System. Thus, those three state parks will have different management than other California parks, management consistent with both state and federal regulations and policies. Those state parks will most likely continue to be managed as administratively withdrawn areas, reserved from timber harvest (except thinning), and thus classifying them as administratively withdrawn is not as inaccurate as one might assume. Also, Moeur et al. in review, classified 1,025 acres as harvested for timber within these boundaries, which is incorrect. According to park officials, only about 35 acres have been cut during the assessment period (and little has been thinned). Does that error in the mapping affect the interpretation of murrelet habitat changes in California?

**REPLY:** We added a sentence to explain the implications of this mapping error. In summary, these state lands are managed as if they are federal reserves, so our interpretations are still valid. The reviewer is not correct in stating that 1,025 acres was harvested within these boundaries; our data indicate very few acres of harvest here.

p. 10. Under reserved lands. Perhaps refer to the Fig. 3 that goes with Chapter 1. Currently, each chapter is independent of the next with its own numbering of figures. The chapters could be more integrated with less duplication of figures. For example, Figs. 1 and 3 in this chapter do not provide much new information, other than the elevation masks, which are already presented in Table 5 in chapter 4.

**REPLY:** We will need to adjust duplicate figures in the final round of edits. We left these in the current draft, but they can be taken out if the editors find unnecessary duplication.

p. 12. Under occupied stands. Polygons are defined as forest stands of relatively homogeneous cover type (does that mean similar tree size?), and then as sites, which I assume are polygons within forest stand polygons in many cases. So when it says the team "randomly selected a subset of these polygons", are the polygons the stands or the sites? Also, why were the sites selected the same as those analyzed in Chapter 4 (except for the addition of western lowlands in Washington)? I assume that the analysis in Chapter 4 was constrained by a budget of obtaining on the ground data, yet in this chapter, data were collected using GIS. With over 10,000 stations to choose from, the set of occupied sites selected for analysis seems small (and no occupied sites were included in the most southern end of the plan area).

**REPLY:** We clarified terminology to indicate we are working with Polygons, and we did randomly select polygons. The individual stations are not suitable as training sites, as there is no assurance that the locations of these stations correspond to the locations of the birds that may have been observed. That is the reason we selected polygons, as these include the areas known to be used by the birds.

p. 13. First full paragraph. Indicate which physiographic provinces with potential murrelet habitat are not represented.

**REPLY:** Done.

p. 14. Good use of nests for validation!

**REPLY:** Thanks!

p. 15. Do the elevation masks consider the more recent findings by the Washington DNR of occupied sites at very high elevations in Washington? Also, some areas are at appropriate elevations, but have little fog, and are not used by murrelets (e.g., eastern Siskiyou National Forest, most of Six Rivers National Forest and the Klamath National Forest). Ninety-eight percent of occupied plots were within fog-influenced vegetation zones in California and southern Oregon (Meyer et al. 200). The rest of Oregon may have a fog limitation, too. The fog effect should be accounted for by including a fog variable in the model or using a fog-zone mask in the appropriate areas. Unfortunately, ENFA does not use binary variables very well (e.g., in or outside fog-influenced vegetation zone), which is a disadvantage of that model compared to logistic regression.

**REPLY:** The DNR sites are not necessarily occupied, and these also were taken after the range of dates we considered in our database. The issue of the inland range of the murrelet is a good one, and we recommend a thorough analysis to refine the eastern extent of the range. We used a distance to sea variable to help evaluate the importance of sites further inland. The reviewer is correct in stating that ENFA does not handle binary variables. In any case, there is no available layer that denotes the “fog zone.”

p. 17, top. It would be helpful to mention here that the CALVEG map was resampled at 100 m pixels to make it more comparable to the IVMP map, according to Moeur et al. in review. Did you use those resampled maps?

**REPLY:** We actually rasterized the CALVEG map at 25-m resolution for use in the ENFA models.

p. 17 Expert Judgment Model. Expert judgment comes from experience in the field, examining data, or reading the literature for a species. It would be nice to have a short discussion in this chapter of the reasons experts chose these classifications, particularly why large dbh hardwood-dominated forests were classified as high suitability, when hardwoods have not been found to be used for nests (as discussed in Chapter 2). In

Chapter 4, only coniferous trees were measured, and broadleaf cover was negatively associated with habitat in the ENFA models. Also, dbh-platform density relationships vary by species (see Chapter 4), and thus, the tree species probably needs to be considered in the expert models, not just the dbh and structure. Second-growth redwood may be very large, as large as some old-growth Douglas-fir or western hemlock, yet not have developed as many platforms as old-growth Douglas-fir or old-growth redwood. The separate ENFA models for each state probably avoided this problem. Also, is the quadratic mean dbh based on tree canopy cover or tree density? Is it for the dominant and co-dominant trees?

**REPLY:** We added some text to address this point. We did not, in contrast to the reviewer comment, assign higher value to broadleaved stands. We agree that tree species would be a good variable, but we do not have species data for the IVMP dataset (Oregon and Washington); that data includes only conifer or broadleaf designations. We added text to clarify the QMD definition.

p. 19, Last sentence of first partial paragraph is unclear. What landscape metrics were calculated using APACK--patch size? Explain what is meant by patches were defined based on a neighborhood of 8 adjacent cells. Weren't patches defined as groups of contiguous 100-m grid cells coded as the same class?

**REPLY:** We added text to clarify this point. In summary, a pixel was included in a patch if any of its 8 surrounding pixels touched the patch (corner or adjacent).

p. 20. First paragraph. Overall, I like the idea of using ENFA, a presence only model. However, after looking at the predicted suitability maps, I realized that mapping only occupied sites does not help delineate the boundary of the nesting range because it is unclear if areas where occupied sites are missing were surveyed. In contrast, use of unoccupied sites helps delineate that range (e.g., see Fig. 1 in chapter 4). I think the model predicts too much habitat outside the range where we find nests in Oregon (see Ripple et al. 2003), although it does a much better job than the Expert models. In chapter 4, unoccupied sites were used without hesitation, so I'm not sure why there was a decision not to use them in this chapter for an occupied or unoccupied analysis (at least see if it gives the same results as the ENFA). Also, to reduce the problem of habitat being limited by foraging habitat (third reason given for not using absent sites), it would be good to include more marine habitat variables than just distance to coast (e.g., distance to marine shorelines with high chlorophyll or cold temperatures), which are remotely sensed and available for the plan area.

**REPLY:** If we had a polygon map of the entire murrelet range, then we could use occupied versus unoccupied polygons to build the model and then apply the results to the entire landscape. An attempt was made early on to develop such a map using eCognition software (Moeur, personal comm.) but the attempt failed. As a result, we had no way to apply results of our model to the range, unless we restricted the analysis to occupied pixels. Hence, we took the approach we did. As to foraging habitat in marine water, the

monitoring program was not designed to gather such information. It would be valuable to do so, if managers wish to make the investment.

P. 24, Top of page. It is not clear whether the habitat variables for each pixel were averaged to obtain one value for a stand polygon (did you mean site polygon?) or if each pixel was considered a separate sample (the latter would greatly and perhaps inappropriately increase the sample size).

**REPLY:** We clarified this in the text. Each pixel is analyzed independently. We attempted to include as large a sample of occupied polygons as possible to reduce pseudoreplication, and the sample was drawn randomly from the entire database.

p. 27. Paragraph before Model Applicability. I have a small concern with use of ENFA with Biomapper if the habitat suitability algorithm used was the "median". Distribution of murrelet use on many of the structural variables (e.g., dbh, structure) may not be symmetric, but skewed because murrelets may prefer the largest dbh on the landscape. Perhaps the Box-Cox transformation helps correct some of this, but the distance geometric mean algorithm (described in the Biomapper manual) might be better to use for any future modeling of murrelet habitat with Biomapper.

**REPLY:** Box-Cox does reduce the problem. We attempted to use the geometric mean approach but ran into software and hardware issues that prevented its application in our models.

#### Results:

p. 30. Top. The maps in Figure 5 are beautifully done, although addition of a scale would be helpful.

**REPLY:** We added scale to all maps.

I agree that the expert judgment model appears to overestimate habitat in all regions not only because it does not consider slope, solar radiation, canopy cover, distance to coast, etc., but also because it misses effects of the fog zone, fragmentation of patches, etc. In particular, zone 4 is overestimated, which is also shown in Fig. 14a. The ENFA model seems to overestimate habitat in Oregon (too much inland) and underestimate habitat in California on private and non-federal land. It also misses some coastal habitat on state park lands and on private lands around Point Arena, perhaps because no occupied sites down there were included in the dataset.

**REPLY:** Our new models should have corrected much of this issue, and seem to better represent the areas the reviewer highlights.

Restricting mapped murrelet habitat to Zone 1 makes sense because there is not much support that they are nesting in Zone 2. The K-fold crossvalidation technique showed good accuracy of the maps and I like its use for validation, yet it does not show that farther inland areas repeatedly surveyed and not used by murrelets were classified as

suitable, such as the eastern side of the Siskiyou National Forest. The next step may be to overlay all occupied and unoccupied sites ever surveyed on these maps to assess if some large areas classified as suitable are rarely used by murrelets.

**REPLY:** This is a good suggestion for future work. As noted above, we believe a thorough evaluation of the murrelet's inland range is warranted.

p. 32. Moeur's personal estimate seems high of 15% per decade compared to the 7.7% given for >20 inches over the last decade in Moeur et al. in review. Why is that?

**REPLY:** The difference is that Moeur actually provided us a separate table of transition rates for the lands in the murrelet range, whereas her reported results apply to a much broader area of the entire NWFP.

p. 33. Line 16. As forest matures, it usually accumulates a higher loading of flammable fuels, and susceptibility to fire. Is this true for the study area? The rate of loss to fire may increase in areas where the forest is becoming murrelet habitat. Also, potential losses to disease are not considered. Ideally, a historic range of variability study on old-growth percentages that occurred in this area as well as historic frequency and extents of disturbances such as fire and disease could help put the current murrelet habitat and potential future changes into perspective (has there been such a study completed?).

**REPLY:** We added a sentence to address this point.

p. 35. First full paragraph. You might add that the Western Washington Lowlands have many occupied sites (see Fig. 2), which indicates there is habitat in those areas, although it may not be in high quantity. Are those highly fragmented forests that may be abandoned in the future? Would that habitat be of lower quality if a time lag effect were included in the model (see Meyer et al. 2002)? I think these are important questions to address in the future.

**REPLY:** We added a sentence to address this point.

p.37. Second paragraph. Because trend analysis is a major objective of the monitoring effort, I would hope that analysis of change using the more accurate ENFA model will be of high priority and included within the scope of the next report.

**REPLY:** We hope so too.

p. 38. Top. Mention geographically where the ENFA map is likely conservative in the sense of less likely to miss habitat (overestimates suitable habitat) vs. liberal (misses habitat but more correctly estimates unsuitable habitat).

**REPLY:** We did this later in the document in the discussion section.

## Discussion

p. 39. Under vegetation mapping. Moeur et al. in review gives accuracy estimates that cover the entire plan area (rather than just the Olympic Peninsula province) and those estimates could be given here. Is the 44.5% for QMD classes?

**REPLY:** We clarified this point in the methods section and added a citation to Moeur's full analysis.

p. 39, Second paragraph. It seems to me that the resolution will depend on the grain at which the species perceives the environment. I have found that we humans easily perceive the boundary of stands of very different size from high-resolution aerial photographs that are not easily delineated at fine resolutions of Landsat imagery (which break up one stand into smaller blocks of various habitat types). Whether the murrelets perceive the boundaries of the same stands as we do or key in on finer resolution boundaries, I do not know. But I would hesitate to make such a generalization as to say that finer resolution maps from satellite imagery probably perform better for the marbled murrelet models.

**REPLY:** We agree, but our discussion relates to the performance of the mathematical model, which does better with finer resolution data.

p. 41. Under Model Comparisons. It would be nice to see the model results in Chapter 4 compared to the model results in this chapter, too. Was the purpose of the models in Chapter 4 to help decide which model variables should be used for the models in Chapter 5? This is not clear. I'd also like to see more discussion on how the models can and should be improved during the next monitoring cycle in this chapter (e.g., putting the sites into a spatial landscape context, adding proximity to high quality marine habitat, etc.).

**REPLY:** We added text on how the model might be improved in the next round of monitoring.

p. 42. Expert Judgment Model. I would say the expert model should only be used to assess coarse landscape-level changes in old-growth habitat potentially important to the murrelet.

**REPLY:** We agree.

The ENFA model appears to give a much better estimate of suitable habitat. Note that if a good, predictive model is developed today, then creation of another model in the future to assess habitat change may not be needed, as long as the relationships in the model hold true. For the ENFA models, can one change the values of the habitat variables in areas that are changing and see how the amount of predicted suitable habitat changes? If not, that can be done with regression models.

**REPLY:** This is a good suggestion. No, ENFA can not be used this way, but I suspect we can develop a regression approach based on the results of the ENFA model.

p. 44. Under Relation to Murrelet Population Estimates, second sentence. It is Figure 4.1-2 in McShane et al. (2004), not Figure 5.1-2.

**REPLY:** Correction made.

Note that McShane et al. (2004) estimates almost 5 times as much murrelet habitat on Six Rivers National Forest than Redwood National Park (including the state parks within its boundary). Yet, only a small area on the entire Six Rivers National Forest had occupied behavior during extensive surveys on the Forest between 1992-1997 (and it is questionable whether there were false positives). The ENFA map is much more reasonable for that area. Can you explain which variables in the ENFA model cause the Six Rivers National Forest not to be classified as suitable in California, so future expert models could consider that factor (I'm guessing it is QMD)?

**REPLY:** Yes, QMD is the big factor here due to the large size of redwoods used for nesting.

Table 2. I think the QMD class for IVMP is 10-19.9 but for CALVEG, it is 12-19.9 inches, according to Moeur et al. in review.

**REPLY:** We recomputed the size intervals to match between CALVEG and IVMP.

Table 3. Give sample sizes in this table.

**REPLY:** Done.

## **Reviewer #2**

What is obviously lacking from this report is any serious attempt to synthesize the results, address the most basic questions, and make strong recommendations for ongoing or new monitoring and research. Some of the topics that could be addressed in this chapter include basic questions on Marbled Murrelet populations and habitat that relate directly to the goals of the NWFP. These questions are either not adequately addressed in any of the chapters, or are hidden in the mass of data within the various chapters. These include:

- Are murrelets declining? None of the chapters seriously considers changes that might have taken place in murrelet populations in the NWFP during the past 10 years. The at-sea surveys cover only 4 years and show no trends, but Chapter 3 made no attempt to compare the recent estimates with earlier estimates from other studies. This needs to be done, even if the methods are not strictly comparable (see comments in Chapter 3).
- Is murrelet habitat area changing? This is discussed in Chapter 5 but again with relatively little reference to other estimates of area, or longer term studies within



more localized parts of the Forest Plan area. A stronger attempt to cover other studies would be valuable.

**REPLY:** We include a comparison with habitat estimates made in the FSEIS and with the estimate compiled as part of the recent status review (McShane et al. 2004). We mention that one of the primary reasons for the murrelet listing was loss of habitat. However, we have no other estimates of the historic amount of murrelet habitat, so options are limited in discussing any other data or estimates.

- What methods are recommended for monitoring habitat area? It seems unlikely that the method applied in Chapter 4 can provide long-term monitoring, and the authors of Chapter 5 doubted whether Ecological Niche Factor Analysis could fill that role. They recommended continued use of their Expert Judgment Model for monitoring. Is that the overall recommendation from this entire 10-year review? How might this method and model be improved? What should we expect for future monitoring?

**REPLY:** We make some suggestions about how to “marry” the expert judgment and ENFA approaches for future monitoring.

There is inconsistent and sometimes confusing use of both metric and non-metric measures. Metric measures are the standard in science and the use of non-metric measures (presumably aimed at a few non-scientific readers) reduces the scientific quality and professionalism of the whole package.

**REPLY:** Our direction is to use English units. I don’t know why, except there is a perception that managers don’t understand metrics.

My main criticism is that the authors seem to be too restrictive in their assessment of suitable nesting habitat and focus mainly on the higher ranks of suitable habitat. They do not fully justify their selection of only the highest habitat suitability class (Class 4) in the Expert Judgment model (see point 13) or habitat suitability scores >60 for the ENFA model (see point 15). The selected habitat criteria are not adequately tested on how they compare with real nest sites or real occupied sites. For the ENFA model for example, if one considered habitat with scores >40 this would include 90% of nest sites compared with 68% for scores >60 (Figure 13). To my mind, using scores of >40 is therefore more strongly supported.

**REPLY:** There will always be a problem in defining habitat quality, when quality is assessed on a continuous scale. Obviously, people may disagree on the appropriate cutoff. We display the full range of suitability scores in our tables, so a reader is free to choose another cutpoint if they disagree with our choice. We found that the HS>60 best matched the distribution of occupied sites on the region. This is shown most explicitly in Figure 12 which shows mean scores in each state exceed HS 60 and that known nest sites are 60 and above. By the reviewer’s logic, we could go to HS >0 and include 100% of nests. Choosing a point with 90% of nests is no more objective than our choice.

There is a related problem that habitat areas identified by these restrictive habitat definitions are then compared with much broader habitat categories derived from Chapter 4 (see point 13 below).

A similar apparently arbitrary decision was to consider patches >1000 acres (about 400 ha) as large patches. How was this justified? Why not consider 500 acres as a large patch?

**REPLY:** We added another column to Table 6 that shows the amount of area in patches of 500 acres.

There is rather limited reference to previous habitat area estimates, and no quantitative comparison between the habitat areas estimated here and those estimated by previous studies (excluding Chapter 4).

**REPLY:** We do compare our estimates with the earlier FSEIS and with McShane et al. 2004 (status review).

### **Specific comments on Chapter 5**

1. Introduction and methods seem thorough and clearly written. Thanks!

2. p. 6 near bottom. Explain the acronym FEIS.

**REPLY:** Done.

3. p. 10 line 4. Explain Matrix allocations. Is this what is available for timber harvest?

**REPLY:** Yes. We added text to make this clear.

4. p. 12 line 9-10. Does Marbled Murrelet range as cited here include the inland Zone 2 shown on Fig. 1. Make this point explicit.

**REPLY:** Done.

5. p. 13 line 5. Singer et al. (1995) seems a rather restricted and somewhat outdated citation for such general statements, presumably applicable to the species entire range. Better to cite McShane et al. (2004) or other more recent reviews.

**REPLY:** Done.

6. p. 17 near bottom. Who were the members of the modeling team who made the expert judgments – was it the authors of this chapter? Be explicit.

**REPLY:** Done.

7. p. 19 end of para 1. Give some indication of the spatial size of the cluster of 8 adjacent cells.

**REPLY:** Done.

8. p. 20 lines 8-10. Comment only - These statements reduce the validity of the analysis made in Chapter 4.

9. p. 21 bottom. It might improve understanding of the factor analysis to make explicit references to the graphs and symbols in Figure 4; e.g., does the marginality factor mean  $\mu_G - \mu_S$  ?

**REPLY:** Text added to make this clear.

10. p. 23 lines 9-12 vs. 12-15. It is difficult to understand the difference between these two calculations of habitat suitability scores.

**REPLY:** This was a typo that has been fixed.

11. p. 23 last paragraph. Here you discuss “presence” when you really mean “occupancy” in the context of Marbled Murrelet surveys. This creates confusion – best to avoid the term presence.

**REPLY:** We agree; change made.

There is also confusion in some places over the application of the “global” designation. In some situations it appears to be applied to data from the state and in other situations to data from a physiognomic province. Depends on what area is being analysed. In the caption to Fig. 4 it mentions state and province in the same sentence which I assume is an error.

**REPLY:** We fixed this and have made a better effort to use consistent terminology.

12. p. 27 line 3 from bottom. I think you mean Figure 2 here.

**REPLY:** Yes, change made.

13. Selecting habitat classes for comparison. I have some problems with the way in which habitat suitability classes were selected for comparisons. It is obviously an arbitrary decision to select only the highest class (Class 4) as representative of murrelet habitat. I feel this selection is unnecessarily restrictive and not sufficiently explained or justified. The actual range of nest sites falls within a wider range of habitat quality (see Figure 13). Including Class 3 appears to increase the area of suitable habitat by 12% (data in Table 12a) – is this not a more defensible measure of murrelet habitat?

**REPLY:** See the response above.

The problem is that the comparisons made between habitat area estimates (p. 29-30) appear to be based on rather restrictive definitions of suitable habitat from this study (Class 4), but much more general definitions of habitat from Huff et al. Chapter 4 (>50% probability of occupancy). The comparisons are therefore rather meaningless, and not a good way to assess the different models.

**REPLY:** That is why we also display (in tables and figures) the amounts of land in all suitability classes. A reader can elect to choose a different range of values if desired.

14. p. 33. Move the explanation of LSR from line 8 up to line 1 where it is first mentioned.

**REPLY:** Done.

15. p. 38 and Table 8a & b. Again there is an unjustified focus on habitat which is >60 or >80 on the habitat suitability model, and the boundary between likely habitat and unlikely habitat seems purely arbitrary. If you consider habitat with scores >40 this would include 90% of nest sites compared with 68% for scores >60 (Figure 13) and probably a similar very high proportion of occupied sites.

**REPLY:** See response above. If we went even lower we could include 100% of nests. Does this make sense?

I don't agree that a selection which leaves out a third of the nest sites and a third of occupied sites gives confidence in the use of scores >60 as stated on p. 38. You can do better, as shown above.

16. p. 40 middle. It would be useful to give a list of the behaviors used to classify occupied detections (this should probably be in the methods section). Be sure to clarify whether circling above the canopy is included or not, because this behavior is sometimes included as occupied.

**REPLY:** Done.

On same page. You should also discuss evidence for surveys which fail to detect occupancy when there are in fact murrelets in the stand. See Cooper and Blaha (2002) for evidence from radar studies, and also Stauffer et al. (2001, 2004).

**REPLY:** Done.

Cooper, B. A., and R. J. Blaha. 2002. Comparisons of radar and audio-visual counts of marbled murrelets during inland forest surveys. *Wildlife Society Bulletin* 30:1182-1194.

Stauffer, H.B., C.J. Ralph, and S.L. Miller. 2001. Incorporating detection uncertainty into presence-absence surveys for marbled murrelet. Covello, CA, Island Press.

Stauffer, H.B., C.J. Ralph, and S.L. Miller. 2004. Ranking habitat for Marbled Murrelets: new conservation approach for species with uncertain detection. *Ecological Applications* 14:1374-1383.

17. p. 41 line 5. What are these likely biases and what effects might they have on estimates of habitat availability?

**REPLY:** Added text to mention some of these.

p. 41 end para 2. It is helpful to provide cautions of this type to avoid misinterpretation of the results of the modeling. Thanks.

18. p. 43 line 1. Provide a references to support this statement on the Douglas-fir stands.

**REPLY:** Done.

19. p. 44 end para 1. You've identified problems with repeating the ENFA analysis as a monitoring tool, but provide no alternatives here. Perhaps mention that future monitoring is discussed on p. 45 so that the reader is not left wondering what you recommend.

**REPLY:** Done.

20. p. 44 middle – should be Figure 4.1-2 (not 5.1-2).

**REPLY:** Change made.

21. p. 45 bottom. Nice to see the analysis identify problematic gaps in murrelet data. Thanks.

22. Literature Cited. These papers in the Lit. Cited do not appear to be in the text.

**REPLY:** They were there – the reviewer missed them.

- ESRI 1992-2002.
- Meyer 1999
- US Fish & Wildlife Service 1997

Don't need first names for Evans Mack 2003.

**REPLY:** Fixed.

Need place and publisher for IVMP 2002.

**REPLY:** Removed this reference and replaced with Moeur et al. in review.

23. Tables

- Avoid using a and b for Table numbers. Just rename Table 4a as Table 5. Same thing for 8a and 8b.

**REPLY:** We renumbered the tables as suggested.

- Table 1. Explain the acronyms used in column 1 (PF, MSS-C etc.)

**REPLY:** Done.

- Table 6 needs a more detailed caption to explain what these data are and what they mean. E.g., explain that the Spearman correlation and CV refer to the K-fold validation. Explain what marginality, specialization and tolerance mean.

**REPLY:** Done.

- Table 7. Caption says Remaining numbers in parentheses are factor loadings. I don't see these in the table.

**REPLY:** Made a change to clarify this.

- Table 7. There doesn't seem to be any logic to the order in which the Factors 1-4 are ranked - Factor 2 explains a larger % than Factor 1.

**REPLY:** BioMapper is different than the usual factor analysis, and the first factor may or may not explain more variance than the remaining factors.

24. Figure 4 caption. It would be useful to mention that this diagram applies to the ENFA model. Again perhaps use the term murrelet occupancy rather than murrelet presence to avoid confusion with these terms.

**REPLY:** Done.

25. Figure 12. Minor point. Any explanation why nests in CA show slightly poorer habitat suitability than occupied sites whereas the reverse is true in OR and WA?

**REPLY:** Not the case with new results.

### **Reviewer #3**

Reviewer #3 did not have comments on this chapter.

## **Summaries and Abstract**

### **Reviewer #2**

### Specific comments

1. p. iv line 6 from bottom. “A substantial amount” is very vague – at least give an approximate estimate of the %.

**REPLY:** Fixed to “high proportion”

2. p. vii point 4. Does this mean 6 years in addition to the four already done or just 2 more years to reach 6? Same thing for the 9 and 15 years.

The number of survey years relative to % change which is reported here is somewhat inconsistent with that reported on the top of page ix.

**REPLY:** fixed

3. p. ix Major findings first point. This statement seems somewhat contradictory saying this is the first estimate but then saying this is better than previous estimated. Perhaps re-word to avoid that perception of contradiction.

**REPLY:** reordered text to clarify

### Synthesis chapter

What is obviously lacking from this report is any serious attempt to synthesize the results, address the most basic questions, and make strong recommendations for ongoing or new monitoring and research. Some of the topics that could be addressed in this chapter include basic questions on Marbled Murrelet populations and habitat that relate directly to the goals of the NWFP. These questions are either not adequately addressed in any of the chapters, or are hidden in the mass of data within the various chapters. These include:

- Are murrelets declining? None of the chapters seriously considers changes that might have taken place in murrelet populations in the NWFP during the past 10 years. The at-sea surveys cover only 4 years and show no trends, but Chapter 3 made no attempt to compare the recent estimates with earlier estimates from other studies. This needs to be done, even if the methods are not strictly comparable (see comments in Chapter 3).
- Is murrelet habitat area changing? This is discussed in Chapter 5 but again with relatively little reference to other estimates of area, or longer term studies within more localized parts of the Forest Plan area. A stronger attempt to cover other studies would be valuable.
- What monitoring methods are recommended for tracking populations in future? There is clearly a commitment to continuing at-sea surveys as the primary method of tracking populations, but there is surely scope for including more reliable radar surveys for tracking populations in selected areas.

- What methods are recommended for monitoring habitat area? It seems unlikely that the method applied in Chapter 4 can provide long-term monitoring, and the authors of Chapter 5 doubted whether Ecological Niche Factor Analysis could fill that role. They recommended continued use of their Expert Judgment Model for monitoring. Is that the overall recommendation from this entire 10-year review? How might this method and model be improved? What should we expect for future monitoring?

**REPLY:** A statement about the synthesis report was added to Chapter 1

The Abstract and Executive Summary seem clearly written and adequate. They are, however, somewhat devoid of actual data, especially from Chapter 5.

**REPLY:** data from results were added